

# SCIENCE

NEW YORK, AUGUST 26, 1892.

## IS THERE A SENSE OF DIRECTION?

BY J. N. HALL, M.D.

ALTHOUGH it seems to me beyond dispute that among the lower animals there is an instinct which teaches them to find their way to a given point regardless of darkness or of previous knowledge of the locality, I do not believe, as I formerly did, that man possesses a similar sense, if we may so term it. I believe that man's ability to find his way to a given point is dependent solely upon a habit of observation, almost unconscious, to be sure, in many cases, but necessary to the end in view. I shall not discuss the truth or falsity of the ingenious theory advanced a few years ago, that the pineal gland in the brain is the seat of such a sense in animals, and that they find their way by means of some perception by this portion of the brain of the direction of terrestrial electric currents. All reasonable men, I believe, are satisfied that animals have this ability to find their way. Thus, most of us are familiar with instances in which a cat, for example, has been taken in a box or satchel for ten, twenty, or even fifty miles from home, and has returned in such an incredibly short time that we may be certain she has travelled by the most direct route. Carrier pigeons transported in closed cars or in ships have no difficulty in determining their direction of flight, even when liberated out of sight of land. I have repeatedly, when in doubt as to my direction upon a prairie without roads or paths, given my pony his reins, as riders commonly do in such circumstances, and never yet knew one to come out at the wrong place. The cowboys of this region make it a rule to pick for night-herding well-broken horses that are known to be anxious to reach camp when given the reins. Such ponies, even if obliged to follow the herd away from camp for several miles, will find their way back in safety in spite of the darkness. This selection of certain horses for night-work does not in the least vitiate our conclusion. They are not chosen for their power of finding their way back, but for their known inclination to do so. Even these horses sometimes fail, as, for instance, in the face of a severe storm, for they drift with the wind at such times rather than face it. Thus I once started for home at midnight from a ranch four miles away. For the first mile my road led westward to a road that ran in a northerly direction to town. Upon this first portion, with nothing to guide him, for it was dark and the ground was covered with new fallen snow, the horse found his way easily. As I struck the road and turned his face fairly to the storm, he would hardly face it. As the thermometer fell to 27 below zero that night, and the wind was strong, it was not strange. In this case the pain in his eyes from the cold and the driving snow more than counterbalanced his desire to get to his stable, and so he preferred to drift with the storm rather than face it.

As I cannot conceive that a horse or pigeon should guide himself by the position of the sun or of the north star, even if we eliminate from the problem the well-known fact that darkness seems to make no difference in the exercise of this

homing instinct, I think that we may take it for granted that animals and birds have this sense of direction, for examples similar to those given above might be given by the score. It might be supposed that this instinct had formerly existed in man, but had been lost during his progress toward his present state of civilization. Writers speak of the "unerring instinct" which guides the red man through the vast stretches of pathless forest in which he resides. But we are also told of the accuracy of observation of the individuals of this same race. The Indian is familiar with the path of the sun and the position of the heavenly bodies. He observes every thing within his horizon, the mountain ranges, prominent peaks, and passes; he notes every stream, its size, character, and general course; he sees all the prominent objects along his trail. If the sun is obscured, and he is temporarily lost, he accomplishes his orientation by observing the rougher bark on the north side of some varieties of forest trees; or he finds the wild morning-glory facing eastward at day-break, for the faithful Moslem is not more certain to look toward the rising sun. He no doubt observes, also, that the warping action of the sun's rays detaches the bark sooner from the south side of the standing dead timber than from the other sides. These and a hundred similar signs are to be read by the student of nature. Such a student, most emphatically, is the Indian. I have had occasion to note his wonderful powers of observation, and those more familiar with his habits than I am, inform me that only after years of experience, if at all, does the white man acquire his proficiency in this direction. We are told by travellers that it is much the same with other primitive races, the necessary qualities being intensified by inheritance through long generations of nomadic ancestors. But as we have advanced in civilization, and sign-posts have taken the place of the signs which the Indian reads, we have retrograded in these matters until the civilized man, despite his knowledge, is lost more easily than his barbaric ancestors, unless he takes especial precautions to note those things which they observed without effort.

It seems to me that our proposition, viz., that we keep our direction by observation, conscious or unconscious, of surrounding objects, will be established if we are able to prove these three things:—

First, that those lacking in the power of observation are most easily lost.

Second, that those in whom this faculty is well developed are rarely lost.

Third, that the latter are easily lost when they lose sight of all external objects, as in fog or darkness, or when their attention is concentrated upon something else to such an extent that they do not observe their surroundings.

I trust that my term "power of observation" is plain to all. In this connection I mean that faculty which enables one to note surrounding objects, and to bear in mind their relations to each other and to himself. I take it that the power which enables one to look at a landscape and say that it is familiar is the same as that which permits some of us to look at a word and determine whether or not it is spelled correctly; for I have long believed that notoriously poor spellers were such, not from poor memories necessarily, but

from lack of the faculty in question. Thus I have a friend with whom I have hunted on several vacation trips to the Rocky Mountains. He has an excellent education and a memory far better than the average, but is utterly unable to spell. He is the only man with whom I ever hunted who was afraid to hunt alone in a strange country for fear of getting lost. I have often been struck, in other matters, with his same deficiency in this direction. Thus, when we hunt together, he scarcely ever sees the game first, although when discovered at a distance, he is immeasurably my superior in determining what class of game it is, if so far off as to render this a matter of doubt.

This example I may count as the first point in establishing our first proposition. Next to observers poor by nature, we might place those who lack experience, as those who have always dwelt in cities. Of course the great majority of these acquire proficiency by practice. Short-sighted persons who do not correct their myopia by the use of glasses come under the same head, for, being unable to observe their surroundings, they are very prone to become lost. Fortunately this disease is comparatively rare in primitive races, natural selection, no doubt, contributing to render it so, for it is vastly more common in civilization.

Among the female inhabitants of towns and cities the faculty in question has had no opportunity for development for many generations, perhaps. They ordinarily have a very poor "sense of direction." I have yet to see a woman from civilized life who could be trusted to point out the way across a pathless region of any considerable extent.

Second, good observers do not readily lose their way. My experience in this regard has been largely with two classes of men, hunters and cowboys. Men of either of these classes, to be even moderately successful, must be the closest of observers. The appearance of a man or an animal anywhere within the circle of vision is ordinarily noted at once. The habit of seeing what lies before one, a thing not given to us all, is formed. With men who travel much alone, the exercise of this faculty fills the gap left by the lack of opportunity for conversation. It gives the mind a certain amount of exercise. The Mexican sheep-herder who is alone on his range will tell you, a week after, who has passed by, what kind of a horse he rode, whether a colt followed a certain wagon the trail of which he has seen, and other details that surprise one not accustomed to such matters. The cowboy who rides a hundred miles across country will tell you the brand of every stray steer he has seen. These men, realizing that they are dependent upon their own exertions for safety, unconsciously develop those faculties of service to them. Other men, placed in similar positions, develop in the same manner, as trappers, explorers, and scouts. Think, for instance, what chance there would be of a trapper's getting lost when he is able to place fifty traps in a new region and find them all without effort. Here his memory is, of course, of as much importance to him as his close attention to his surroundings.

Our third proposition is, that even those who are ordinarily entirely competent to find their way get lost easily in darkness, fog, or snowstorms, and especially if interested in something which thoroughly occupies the mind. This I believe to be utterly inconsistent with the theory of a proper "sense of direction." Examples are, no doubt, familiar to all, but I will quote one from my own experience, which to me is conclusive. I have for years been in the habit of hunting alone in my vacation trips, upon the plains as well as in the mountains, and have travelled much in unsettled

districts, both night and day. Realizing the possibility of getting caught in a snowstorm, I have made it a rule to carry a pocket compass as well as a waterproof match-safe at all times. For eight years I never had occasion to use the compass to learn my position, and I almost believed I was infallible so far as the question of getting lost, in daylight at least, was concerned. But the undeceiving came, and it was that which led me to this study of the subject. One fine September day I started out from camp on a deer-hunt. We were in the part of Wyoming between the headwaters of Savory and Jack Creeks, about two miles from that portion of the Continental Divide which lies between them.

Within half a mile of camp I struck a deer trail and followed it. I pursued it for two or three miles, mostly through heavy timber, without seeing any signs of game, although momentarily expecting to do so. When I finally stopped for a moment, it had begun to rain, and the dense clouds shut in every hilltop. I could see nothing to indicate the position of the sun, and there was not a breath of wind. The rain increasing, I decided to start for home, and, turning farther to the right, followed, as I supposed, a tributary of Jack Creek down into the valley. What was my consternation to find that the creek into which it led flowed to the right instead of to the left as Jack Creek should do! Every thing was unfamiliar. I had crossed no ridge, to my knowledge, high enough for the Divide; I was dumbfounded. I knew, however, that I was upon the westerly side of Jack Creek, for I had crossed no stream of any description. In two hours I could not possibly have walked far enough up or down to cause me to miss it if I adopted an easterly course. The difficulty was in the fact that I had supposed that I had been following such a course in arriving at my present position. As the mist and rain now shut in every thing, I had nothing to do but to complete my humiliation by a forced resort to the compass, for I had to admit for the first time that I was lost. At first sight I was tempted to believe that the needle was wrong, as I am told all men in similar position are. I carried the compass to some distance from my rifle, fearing that the needle was deflected by the metallic barrel. The result was the same. Fearing that I had found a body of iron ore by accident, I tried various localities, but the needle still persisted in pointing, as it seemed to me, south. After a few moments' consideration I started over a ridge a little to the right of the way I had come, and due east by compass. I still felt that I was going west, and could not get over the idea. A tramp of half an hour brought me within sight of the valley I sought, and north seemed to come around where it should have been all the time. I had unconsciously crossed the Divide at its lowest point, far lower than the one at which I now crossed, evidently having made an entire turn when starting homeward instead of a half one as I had intended. I now made a bee-line for camp, but I carried home with me less faith in my "sense of direction" than I had upon starting out.

I might quote from the experience of others a dozen similar examples of losing one's way. Some seven or eight men have been more or less severely frozen in this very county, by losing their correct route. I believe that further examples are unnecessary. It is sufficient for me to say, in conclusion, that, whatever instincts man may have had in a former state, he has at present no means of finding his way at all resembling that possessed by birds and animals.

Sterling, Col.

## CURRENT NOTES ON ANTHROPOLOGY. — XIII.

[Edited by D. G. Brinton, M.D., LL.D.]

## The Primitive Carib Tongue.

THE expedition led by Dr. Karl von den Steinen, which explored the head-waters of the Schingu River in Brazil, made some remarkable discoveries. Tribes were found who had never heard of a white man, and were utterly ignorant of his inventions. They were still wholly in the stone age, uncontaminated — the word is not misapplied — by any breath of civilization. In ethnography, the most interesting find was the identification of the Bacahiris with the Carib stem, and apparently its recognition as perhaps the nearest of any of the Carib tribes to the original stock.

Dr. von den Steinen has just issued his linguistic material obtained from this tribe in a neat octavo of 403 pages, "*Die Bakairi-Sprache*" (K. F. Koehler, Leipzig, 1892). It contains abundant sources for the study of the group, vocabularies, texts, narratives, grammatical observations, and, what is peculiarly valuable, a close study of the phonetic variations of the various Carib dialects as far as they have been ascertained. He shows that in all the associated idioms the same laws of verbal modification hold good, although each has developed under its own peculiar influences. The thoroughness which marks throughout this excellent study places it in the front rank of contributions to the growing science of American linguistics.

## The Ethnic Distribution of Roofing Tiles.

As a floating leaf will indicate the current and eddies of a stream better than a floating log, so oftentimes a humble art will be a more accurate indication of the drift of civilization than the more ostentatious products of human ingenuity. This has been happily illustrated by Professor Edward S. Morse in a paper "*On the Older Forms of Terra-Cotta Roofing Tiles*," published in the *Essex Institute Bulletin* for March of this year.

He finds that the older roofing tiles of the world group themselves into three distinct types, the normal or Asiatic tile, the pan or Belgic tile, which is an outgrowth of the normal tile, and the flat or Germanic tile, which is an independent form. The geographic areas in which these various tiles are found and the history of their distribution are reliable indications of the conquest or peaceable advance of certain forms of civilization. Professor Morse's paper is abundantly illustrated, and an interesting map is added, showing the present distribution of the three types of tiles over Europe, northern Africa, and western Asia.

That his study may not remain one of archæology only, the author adds a number of practical hints on the use and value of terra-cotta tiles as roofing material, and suggests their wider introduction in the United States. They offer the best of all roofing material, durable, fire-proof, cheap, decorative, warm in winter, and cool in summer.

## Celts and Kymri.

Professor Topinard is not satisfied with the result of the discussion of the Celts in *Science*, March 11, 25, etc. He takes it up in *L'Anthropologie* for June, and draws a distinction between the Celts of the "men of letters," among whom he classes the editor of these "Notes," and the anthropologists, represented — by himself.

"For the former," he says, "the Celts are blonds, they constructed the megalithic monuments, and spoke a language now unknown. For the latter they are the brachycephalic people of western Europe, who appeared at the neolithic

epoch, and lived during the age of bronze side by side with those who later bore the name of Gauls. . . . For Broca, the term Celt designated the brachycephalic group of western Europe, and the term Kymri the blond group, with long and narrow face, etc. We retain the meaning he gives to Celtic, but to meet certain objections substitute for the word Kymri that of Gall or Gaulois."

As the opinion of Broca to this effect was quoted with approval in the discussion (see *Science*, April 22), it is difficult to perceive the grounds on which the learned Parisian professor makes his objections. But it is desirable that his own views, which are always worthy attentive consideration, should be presented.

## Architecture as an Ethnic Trait.

The significance of architecture as an ethnic trait has been fully recognized — too fully at times — in reference to the domestic architecture of the American Indians. The views of Mr. Lewis A. Morgan, who could see nowhere on the continent other than "long houses" and "communal dwellings," contained a genuine discovery which has been pushed at times beyond its reasonable limits.

Some excellent articles on this subject have appeared from time to time from the pen of Mr. Barr Ferree, in the *American Naturalist* and the *American Anthropologist*. He treats such subjects as "The Sociological Influences of Primitive Architecture," and the climatic influences which have given rise to this or that peculiarity or style. His essays are thoughtful and well reasoned.

In the first fascicule of the *Bibliothèque Internationale de l'Alliance Scientifique*, M. César Daly pursues this train of thought to the point of announcing — "given a social condition, it will have such a religion and such an architecture." In regard to "styles," he discriminates between that of the architect, which is transient, and that demanded by the tastes and requirements of the community, which depends on it alone and will last as long as these remain. "A style in architecture is therefore something national, social, and religious, and not royal, as that of Louis XIV., nor that of an artist, had he all the genius in the world."

## Types of Beauty among American Indians.

In a note published in this series (*Science*, June 3), attention was directed to the power of beauty in developing the race toward a certain standard of physical perfection. Some interesting facts bearing directly on this topic are presented by Dr. R. W. Shufeldt in a recent pamphlet on "Indian Types of Beauty."

He begins with the suggestive remark that men of the lower type of development cannot perceive the beauty in the women of the higher type nearly so readily as the men of the higher type can recognize the comeliness in the women of the lower. This is as we might expect, the education in the elements of the beautiful being principally a result of development.

Dr. Shufeldt inserts a number of photographs of Indian beauties, an inspection of which will satisfy any one that the opinion which in their own tribe awards them the palm for good-looks is justified by all standards. The same fact is borne out by Mr. Power in his work on the Indians of California. He calls attention to the attractive appearance of the maidens of several tribes reputed among their own people as beauties.

While in all stages of civilization there are false and abnormal standards of the beautiful — notably so among ourselves — there is also a gradual and certain tendency toward

that ideal of physical form which the keen artistic sense of the ancient Greeks recognized as the perfection of corporeal symmetry. Wherever it is present in any degree, it is sure to be recognized. As Novalis says in one of his apothegms, "Beauty alone is visible."

### SOME POINTS IN THE NOMENCLATURE-PRIORITY QUESTION.

BY LUCIEN M. UNDERWOOD.

THERE are some of our younger botanists who see no possible merit in the nomenclature-priority discussion. That this is the case is naturally due to the fact that neither their age nor training have been sufficient to enable them to obtain a general view of botany as a science in which the relations of plants to each other and to other living things form the crowning summit of achievement. When we say *relations*, we mean the word in its deepest and widest sense — morphologic, embryologic, physiologic, geographic, and chronologic.

To those whose work involves the weighing, sifting, and correlating of all the truth concerning some group of plants that has been found out by patient workers in times past and present, as well as that brought to light in their own comparative research, the necessity of some uniform, authoritative, and permanent system of nomenclature needs no argument. If some have acute inflammation of the morphologic nerve so that their attention is largely drawn away from the general wants of the system to the nursing of their peculiar member, they are worthy of our sympathy, but they must reduce their hypertrophy before they can expect the botanical world to regard their judgment as normal outside their special sphere.

While we thoroughly believe in Goethe's assertion that "species are the creation of text-books while Nature knows only individuals," we have not yet advanced sufficiently far to be able to discontinue the present method of grouping individuals into species and recognizing them by certain fixed names. This is a matter of convenience, and it is a present logical necessity. We believe, therefore, that the matter of nomenclature ought to be settled at once and permanently, and this we believe to be the opinion of all who look at systematic botany, not as a mere "battle of synonyms," but in its true position, representing as it does the ultimatum toward which every fact in the science tends, and into which the whole science will be ultimately crystallized. So far is this desirable that if a system can be agreed upon, it must and ought to be by the yielding of personal opinions to the will of the best and maturest judgment of the botanical world.

One phase of the question has not yet been sufficiently dwelt upon, and that is the one which involves the element of personal justice. There are some who say that there is no ethical side to the question, that it is a mere matter of expediency. If *justice* pertains to ethics then there is an ethical element in the problem. It has always been maintained that a man has the right to the product of his brain. If he invents a new mechanical contrivance he is awarded a patent. If he writes a book he is given a copyright. If he discovers a new principle or process in the natural world his name is inseparably connected with that principle. Otherwise why do we speak of the Bell telephone, of Marsh's test for arsenic, or of Newton's law of gravitation? The same is true of discoveries in botanical science, for we inseparably connect certain names with the earliest recogni-

tion of protoplasm, the announcement of its identity with sarcode, the discovery of fertilization by antherozoids, the continuity of protoplasm, and every other important addition to a knowledge of the plant world. In the same way the recognition of a natural group of plants, an order, a genus, or even a species is now regarded as of sufficient importance to be credited to the one who makes the discovery, not by any means on the ground of expediency (though it is doubtless in the highest degree expedient), but because of an innate feeling of justice due him who thus publishes the result of his work.

It is true that favored students or organizations may, for a time, regard themselves as the only rightly-appointed medium of description of species, but the multiplication of botanical centres, the specialization of workers, and the growing urbanity and cordiality in extending to specialists the privileges of public and private collections will all tend to prevent the growth of monopolies in a field which is not likely to become narrow enough for any to jostle offensively.

As a worker in one group of plants we present some questions that have suggested themselves in our work, drawing illustrations largely from the genera and species with which we are most interested, seeking not so much to offer dogmatic principles as to call to mind the feature of personal justice.

#### 1. Shall there be an initial date in nomenclature?

What justice on the one hand, or advantage on the other, is there in accepting those of Micheli's genera that were adopted by Linnæus, and rejecting others equally valid that were not? What virtue did the great compiler add to an adopted name that should render it either sacred or immortal? We have *Anthoceros* and *Sphaerocarpus*, *Blasia*, *Riccia*, and *Lunularia*, all established by Micheli in 1729, and all accepted to-day without question, forsooth, because they have received the stamp of the immortal Linnæus, who could scarcely distinguish a hepatic from other Bryophytes. And yet Micheli, the founder of generic distinctions among Cryptogams, who knew and studied plants, adopted other generic names at the same time; these the great Linnæus did not accept because he could not get down to the study of plants and learn to distinguish genera among hepatics and other Cryptogams. Are we of this age so blinded that we must fall down and worship this popularizer of botany and accept his dictum as against that of a man whose shrewdness enabled him thus early to discriminate genera among Cryptogams?

But we must have a starting-point, some say. Why not then commence genera with the men who first originated them? Let us not award merit where merit is not due. Let us not assume for Linnæus a virtue that he did not possess. Micheli, Ruppius, and Dillenius were the originators of genera among hepatics. Why not recognize their genera that represent natural groups? If others are the progenitors of genera in other groups of plants, there is no reason why their work should not also stand, provided their names were not already preoccupied.

#### 2. Shall names long used be laid aside when claimed for other plants on grounds of strict priority? Shall we recognize the principle of outlaw in nomenclature?

For example, *Marsilea* (Micheli, 1729) is a hepatic which since Raddi's time (1818) has been known as *Pellia*. *Marsilea* Linn. has since its establishment been used for a genus of quadrifoliate Pteridophytes. Shall the latter stand in the face of evident priority? While a compromise of this kind,

sacrificing an individual for the general good, if it could be agreed upon by an authoritative body, would be in the interests of both science and peace, it could not be accomplished without personal injustice.

Another case more complicated is that of *Asterella*. This genus was established by Palisot de Beauvais in 1810. Raddi independently established *Reboullia* in 1818. After many years European hepaticologists, with Lindberg at the head, discovered that the two genera were identical; so *Reboullia* yielded to *Asterella*. Meanwhile Nees von Esenbeck had established the genus *Fimbriaria* (1820). Latterly Lindberg took a second thought and regarded Beauvais's three-line description as more nearly representing *Fimbriaria* Nees. So this generic name, known for over half a century, is laid on the shelf and *Asterella*, which we have been using for a totally different plant, is put in its place. On this basis *Reboullia* Raddi was restored.

3. Shall "the first name under a genus" hold against a previous specific name?

*Riccia reticulata* (Gmelin, 1796) was erected into *Corsinia* by Raddi, in 1818, under the name of *Corsinia marchantioides*. Shall this name hold, or shall we write *Corsinia reticulata* (Gmelin) Dumort. (1874)?

We believe the latter more justly covers the case, although on the ground that Raddi's name had been long in use this might be a proper time to sacrifice an individual for the public good!

4. Shall varietal names have priority over established specific names?

Madame Libert described *Lejeunea calcarea* in 1822. It proved to be the same as had been described by Hooker in 1816, as *Jungermannia hamatifolia*  $\beta$  *echinata*. Taylor in 1846 wrote *Lejeunea echinata* Tayl., perhaps more for displaying the caudal appendage than for principle, but he has not been generally followed until latterly, when there is a tendency to revert to his name. Since varieties, especially among Cryptogams, are too often established on mere sports, forms, or other slight variations, and species are the units of classification, we believe that description as a species ought to be the ultimatum in matters of priority. If Madame Libert had recognized the identity with Hooker's variety, and had named it *Lejeunea echinata* in the first place no one would have quarrelled with her, for it would have been advantageous to preserve Hooker's name. Since she named it *L. calcarea* we believe this name should stand.

5. Can inappropriate names be cancelled on that ground alone?

In 1867 Alphonso Wood established a new lileaceous genus from California under the name of *Brevoortia*. Out of compliment to the little daughter of the stage-driver who first showed him the plant, he called it *Brevoortia Ida-Maia*. When Dr. Gray reviewed Wood's species a year later, we deem that he did a double injustice: (1) In hastily cancelling a genus which had not originated at Cambridge, and (2) in substituting a specific name on the ground that the one chosen was a compound. He thus obliterated all trace of Wood's discovery by writing *Brodiaea coccinea* Gray! The first injustice was partly atoned for by Dr. Watson who recognized Wood's genus as valid in his "Revision of the Liliaceae," but instead of writing Wood's name in accord with the principle of "the first name under a genus" he wrote *Brevoortia coccinea* Watson! It might be well to ask why *Ida-Maia* is any more objectionable than *Hart-Wrightii*, *Asagrayana*, *Donnell-Smithii*, or any other of the many compounds of our system.

To take another example, Berkeley established the genus *Cronisia*, closely related to *Corsinia*. Lindberg, not recognizing Dr. Gray's aphorism that "a neat anagram is not bad," cancelled *Cronisia* and substituted *Carringtonia* Lindberg.

We maintain that a name once established cannot be cancelled on the ground of offended personal taste even though it have the euphonious melody and the suspicious flavor of *Mariae-Wilsoni*!

6. How far has a later writer a right to correct names previously established?

We cite three instances:—

(1). In 1821 S. F. Gray established a large number of genera of British Hepaticae. To these he gave personal names *Kantius*, *Herbertus*, *Pallavicinius*, etc. These have been changed by Carrington to a feminine ending *Kantia*, *Herberta*, *Pallavicinia*, etc.

(2). Lindberg has adopted the plan of changing all personal names ending in *ianus*, *a*, *um* to *ii*; for instance, he writes *Jungermannia Helli* for *J. Helleriana* as originally written by Nees.

(3). *Tricholea* Dumort. was corrected by Nees to *Trichocola* to bring it into harmony with its derivation. Dumortier originally wrote it *Thricolea*.

Except in manifest errors of orthography, names should be let alone.

7. What credit should be given for generic and specific names?

(a) Shall we write the name of the author of the specific name in case there has been a transfer to a new genus, and if so in parentheses or not? (b) Shall we write the double combination of the first describer of the species in parentheses followed by the name of the author of the generic combination? (c) Shall we write the name of the one who made the transfer?

While we shall hail with joy the time when the bare binary shall be all that is necessary to identify a plant, we believe the following to represent in a specific instance the order in which the demands of personal justice as well as scientific convenience are most fully met:—

(1). *Metzgeria pubescens* (Schränk) Raddi.

(2). *Metzgeria pubescens* (Schränk).

(3). *Metzgeria pubescens* Schrank.

(4). *Metzgeria pubescens* Raddi.

To write *M. pubescens* Schrank, makes that writer say what he never thought of saying. To say *M. pubescens* Raddi, in accordance with the system long familiar to us by the use of Gray's Manual, is to unjustly transfer the credit of the species where it never rightly belonged, and appears to us the most faulty system of all.

The above questions should be settled by a commission after the example, if not the manner, of the American Ornithologists' Union, if individuals of strong personality can lay aside their peculiar idiosyncracies and unite in a system that will both meet the demands of justice and at the same time serve the highest interests of the science.

To this commission could be referred minor questions like that of "once a synonym always a synonym;" how close may generic names agree in orthography<sup>1</sup>; what form of nomenclature is best for varieties, sub-species and "forms;" and the punctuation and capitalization of specific names. In nomenclature individuality ought to disappear and uniformity universally obtain.

DePauw University, Aug. 15.

<sup>1</sup> For example, should *Richardia* preclude *Riccardia*, or *Caesia*, *Cesia*?

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### THE PROGRESS MADE IN TEACHING DEAF CHILDREN TO READ LIPS AND TALK, IN THE UNITED STATES AND CANADA.<sup>1</sup>

BY ALEXANDER GRAHAM BELL.

THE total number of teachers of the deaf employed in the United States in 1890 was 641, and in 1891, 686. This is an increase of 45. When we come to analyze the details we find that this is an increase exclusively of articulation teachers. This is shown by the following facts. In 1890, there were 213 articulation teachers employed, whereas, in 1891, there were 260,— an increase of 47 articulation teachers. The first statistics upon this subject were collected by the Annals in 1886. In that year we find articulation teachers constituted 32.8 per cent of the hearing teachers in our schools for the deaf. In 1887 they constituted 40.5 per cent; in 1888, 44 per cent; in 1889, 45.7 per cent; in 1890, 45.2 per cent; in 1891, the latest returns, 50 per cent. Indeed, they constituted one more than 50 per cent. There were 260 articulation teachers to 259 hearing teachers who were not engaged in articulation work.

In regard to the proportion of deaf pupils taught speech, the increase during the past year has been very marked. In 1890, there were 3,682 deaf children in the United States taught speech; in 1891, 4,245, an increase of 563. In 1890, 41.3 per cent of our pupils were taught speech; in 1891, 46 per cent. I am sure that this increase is due very greatly to the stimulus of the first summer meeting of the American Association to Promote the Teaching of Speech to the Deaf.

Of course, the statistics in the Annals include the whole of our pupils, old as well as young, and it has occurred to me, therefore, that they may not give us a true indication of the extent to which the California resolution is being carried out in the country at large; and that a better indication would be obtained by statistics concerning younger pupils alone. I therefore sent out a circular letter of inquiry to

<sup>1</sup> Address delivered at the conference of the superintendents and principals of the schools for the deaf of North America, held at Colorado Springs, Aug. 8-11.

the superintendents and principals of American schools for the deaf, requesting:—

1. The total number of new pupils admitted during the school year just closed.

2. The number of new pupils taught speech; and

3. The number of these taught by speech.

Replies have been received from schools containing 7,987 pupils, or 80 per cent of the whole number under instruction in the United States and Canada.

The following table shows the results of the inquiry:—

#### *Speech-Teaching in American Schools for the Deaf, 1891.*

Schools for the Deaf.	Number of Pupils in Year 1891 [Annals of January, 1891].			New Pupils Admitted in School Year Ending June, 1892 [Replies to Cir- cular of A. G. Bell].					
	Total pupils.	Total taught speech.	Percentage taught speech.	Total new pupils.	Total taught speech.	Total taught by speech.	Percentage.		
							Taught speech.	Taught by speech.	
United States.	A...	5,614	2,960	53	836	580	363	69	43
	B...	1,619	759	47	266	189		71	
	C...	1,999	536	27					
Total.....	9,232	4,255	46	1,102	769		70		
Canada.	A...	445	92	21	59	20	5	35	8
	B...	309	132	43	63	41		65	
	C...	39	5	13					
Total.....	793	229	29	122	61		50		

A. Complete returns were received from these schools.

B. The returns received from these schools did not state definitely the number of new pupils taught by speech. The Canadian schools marked *B* refer to the two Roman Catholic schools in Montreal. They return 126 pupils, or 41 per cent of the whole, as taught by speech; but do not state how many of the new pupils were so taught.

C. These schools did not reply to the circular letter of inquiry.

It is encouraging to note that while 46 per cent of the whole number in our schools last year were taught articulation, 70 per cent of the younger pupils were afforded an opportunity of learning to speak. The statistics published in the Annals are somewhat defective because, while they give us the total number of pupils taught speech, they do not give us the number taught by speech; so that we have no statistics by which we can measure the progress of the oral method of teaching in America.

Professor Joseph C. Gordon of the National Deaf-Mute College, in some editorial remarks prefacing a volume entitled "The Education of the Deaf," about to be issued by the Volta Bureau, says: "The returns of pupils taught by speech are incomplete. The number reported for 1891 is 963, or 10.4 per cent of the school population." The above table indicates that the percentage, in the case of the younger pupils, must be very much larger. Out of 836 new pupils admitted during the past school year, 336, or 43 per cent, were taught by speech.

This percentage, however, is probably excessive, because the table shows that those schools which have done the most



work in articulation teaching have been the most ready to respond to inquiries relating to the subject. We cannot, therefore, assume that the percentage holds for the schools that have not replied to my circular letter.

Still, even if we assume that these 363 deaf children were all who were taught by the oral method, the percentage must be very much higher than that given by Professor Gordon. This will be obvious from the following considerations:—

The schools containing these cases had a total attendance of 5,614 pupils, of whom 836, or 15 per cent, were new pupils admitted during the past school year.

If this proportion held good for the whole country, then there must have been a total of 1,385 new pupils—or 15 per cent of 9,232—admitted during the year just closed.

Now 363 of these, at least,—or 26 per cent,—we know were taught by speech. Hence, for the younger pupils, the true proportion taught by speech lies somewhere between 26 and 43 per cent of the whole. The lowest estimate very much exceeds the figures of Professor Gordon.

We have no means of ascertaining whether the proportion of our pupils taught by speech is increasing or diminishing; and I think it would be well to direct the attention of the editor of the *Annals* to the importance of collecting and publishing statistics upon this subject.

I have glanced over the most recent reports of American schools for the deaf, and there are a few points contained in them to which I shall direct your attention.

In the twelfth biennial report of the American Asylum at Hartford, the principal, Dr. Job Williams, gives his views upon what constitutes success in articulation work. He says:—

“We hold that direct and earnest effort should be made, by expert teachers of those branches, to teach speech and speech-reading to every pupil, and in no case should that effort be abandoned, until those teachers are convinced that the pupil will never acquire enough of speech to be of any practical use. In some very unpromising cases the possibility of acquiring speech is not given up for two or three years. Here let me say that the criterion of success in speech should not be perfect naturalness of tone and inflection. It would be unreasonable to expect that, where the sense of hearing is wanting. Intelligibility is the prime requisite of good speech. Tone and inflection are secondary considerations. Any pupil who has mastered speech and lip-reading so far as to be able to carry on conversation in regard to the ordinary affairs of life in speech so plain as to be readily understood by the members of his own family, even though others fail to understand him, should be counted as a successful articulator and lip-reader. It is worth while to continue the instruction in these branches in many cases where the degree of success falls considerably short of the ability to carry on an extended conversation, provided that what of speech is acquired is easily understood. We must recognize the fact that intelligible speech is the readiest and most acceptable means of communication with people in general, but it must be intelligible. It is worth while for a child to gain even a limited amount of speech and lip-reading (the latter is as important as the former) in all cases where it can be done without serious sacrifice in mental development and acquisition of language.”

We all must agree with Mr. Williams in these remarks. We should, of course, aim to have our pupils speak so clearly and distinctly that anyone can understand them; but I am sure Mr. Williams is right in saying that a much lower degree of proficiency might constitute a pupil a successful

articulator and speech-reader. Mr. Williams rightly claims that oral instruction is successful if the speech of pupils is intelligible to their friends in their own homes, and among their own people, even though others have difficulty in understanding what they say. It is a very difficult thing for a teacher, and especially for an articulation teacher, to realize this. I have been myself a teacher of articulation, and I know how they feel. Their ears are sensitive to mispronunciations, as mine were. It is difficult for them to realize that voices, which to them may be disagreeable in tone, may be very sweet and pleasant to those at home. It is difficult for them to realize that imperfect speech may be better than none at all; and that speech so defective as to be unintelligible to strangers, may be of the greatest value to the pupils in their own homes, and among their own people, as a means of communication. This fact has been specially impressed upon my attention by the report of the Mississippi Institution, which, in many respects, is a very remarkable document. That institution has had a class of twelve pupils taught altogether by speech and speech-reading. While all of them have made great progress in speech-reading, some have gained but little power of speech.

Mr. Dobyns, the principal, says:—

“While I have been more than satisfied that the institution was justifiable in the small outlay in this department of instruction, yet, for fear my zeal to keep pace with the times may have gotten the better of my judgment, I submitted the following questions to the parents of the pupils in this class, knowing that they desired the very best thing for their children.”

I will not take up your time by reading the questions and replies, but will merely say that the answers demonstrate, that speech, which may be thought very little of by the sensitive ear of the teacher, is considered a blessing at home. None of these parents desire their children to be removed from the oral department of the school; but, on the contrary, they all earnestly request that their children be continued in this department. Where there is any difficulty in deciding upon the value and success of the articulation taught to our pupils, with whom should the decision rest? Surely with those who are nearest and dearest to our pupils,—with those who have their interest most at heart. Mr. Dobyns, I am sure, is right in referring the question to the parents and friends at home.

In this report, Mr. Dobyns incidentally remarks that now, whenever a new pupil enters the institution, the request comes from the parents: “Please see if you can’t teach my child to speak.” He has, therefore, asked from the Mississippi Legislature an increase of appropriation to enable him to employ another articulation teacher; and I am sure we all hope he may get it.

There is another point in the report of the Mississippi Institution to which I would direct your attention. Mr. Dobyns has collected and published statistics concerning the earnings of former pupils, and he goes to his State Legislature with the proof that the graduates of his school, so far from being dependent upon the public for support, are actually wealth producers, earning annually a larger amount than the State appropriates for the support of the school. He proves that it is not a matter of charity to educate the deaf; and demonstrates that the money appropriated for this purpose is in the nature of an investment, yielding profitable returns to the State.

I would urge all schools for the deaf to carry out this plan of Mr. Dobyns, and collect statistics concerning the earnings

of former pupils. I would suggest that these statistics should be so tabulated as to distinguish the earnings of the pupils who could articulate and read speech from the mouth, from those who could not. I have no doubt that pupils who speak, have an advantage in life over those who do not; and that statistics will demonstrate that their average earnings exceed the average earnings of those who are unable to articulate. If this should turn out to be the case, what an argument it would be to present to legislatures in favor of appropriations for articulation teaching?

I venture to predict, we shall find that our former pupils who speak, even though they may be unable to read speech, earn more per annum than those who are forced to resort exclusively to manual means of communication; and those of them who can read speech, as well as speak, are still better off in life.

Mr. Davidson of the Pennsylvania Institution has suggested another valuable line of inquiry. From a comparison of numerous letters in his possession, he makes the assertion that orally-taught pupils improve in their knowledge and use of language after leaving school. I would suggest the importance of preserving uncorrected letters of your pupils during the whole period of their school life, and of keeping up correspondence with them after they leave school. A comparison of letters written by the same pupil at different periods of time would be invaluable as a means of determining his progress; and the correspondence in adult life might be utilized, for the purpose of collecting statistics concerning the earnings and general success in life of our pupils.

#### REMARKS ON NORTH AMERICAN LICHENOLOGY.— PRELIMINARY.

BY W. W. CALKINS.

In introducing the above title for my subject, I owe to myself and to the promoters and patrons of a journal embracing the scope, influence, and popularity of *Science* an explanation of my purpose in bringing into public notice that department of botany which it appears to me as an humble worker in this field has heretofore received too little attention from botanists and institutions of learning in North America. My object is, then, to contribute in some measure towards the upbuilding of a more general interest among students in what seems to have been considered an uninteresting and obscure field of research.

In other departments there are workers by the hundreds. In American lichenology only one name and one life stands out pre-eminent as the founder, promoter, and able exponent of the science, Edward Tuckerman. He has gone to his rest, but his works remain. As a systematist, he brought order out of chaos. He formulated and developed a classification more nearly approaching Nature in her arrangements and divisions of the Lichens than any previous authors,—unless it be Elias Fries and Dr. Nylander,—both illustrious names.

This system, thus established by Tuckerman, is the basis of the science in this country, and his published writings the sole text-book and guide of the American student. Tuckerman's style of writing is certainly unique,—*sui generis*,—but when once comprehended, impressive and convincing, as well as clear. I confess to long vigils before I could understand him. Having had the benefit of collecting and comparing the greater part of the species described by him in their native habitats, my admiration for his profound knowledge, apprehension, and far-seeing into the secrets of

nature, as evinced by what he calls "habit," increases with each review of his works.

This was made plainer to me from recent investigations in Tennessee, Alabama, and Georgia, by the fortunate finding of several rare saxicolous species which Tuckerman described, and which had not been seen since Judge T. M. Peters discovered and sent them to him. There were doubts in my mind which were now dissipated by an actual review *in situ* day after day, as I wandered over the calcareous rocks of the mountain region where found. I will now only specify one species, *Pannaria stenophylla*, which grows intermixed and cunningly hidden with another but more common form, *Pannaria Petersii*. The thallus and reddish-brown fruit are scarcely distinguishable at first. I am indebted to the keen discrimination of my friend S. Higginson for the complete settlement of this rare species.

Since Tuckerman's death no one has appeared to fill his place; the nearest approach being Henry Willey, who, however, has retired from active work, but not without leaving two publications of great value. In a recent letter to me from Dr. Nylander, he laments these losses to American science. But what has been can be. We must wait for some one of pre-eminent ability and adaptation to grow into the vacancy. Meantime, I doubt if anyone in the United States is making a special study of Lichens. Two or three have considerable knowledge of them, however. This is to be regretted. An inviting field, vast and rich, is open and offers great rewards. Who would exchange a fame like Tuckerman's for any amount of worldly wealth! I apprehend that he himself did not realize the extent or value of his own labors to which his entire life was devoted; neither the gratitude of his followers and successors, who without the works he left would be without a guide, and like an army without a general. I am sure that my co-laborers will agree with me in this. We may then be considered as entering upon a new era in the prospects and progress of the study in this country, which is coincident with the tremendous strides shown in phænogamic botany and in the increasing number of students in cryptogamia—as the fungi. Having myself for many years worked in those fields and witnessed the growth and increasing number of students, I have watched for corresponding interest as to Lichens. From the evidence received by me, the future is promising.

While specialists in Europe have explored every corner, and the great Nylander has given a lifetime of labor to this subject, the species of only detached portions of America have been investigated. The extreme south of our coasts and the far west are almost a *terra incognita*. The sub-tropical portions are prolific in new species and rare forms. It was my fortune to find and submit a large number of these to Willey and Nylander, yet I merely skimmed over the surface. The southern Appalachian Mountain region is almost as interesting in its rock forms, which are the most difficult perhaps to study (*vide* Nylander on my new Tennessee species). Their interest is, however, exceedingly great. While it is true that hundreds of new forms remain to be discovered, and are a great incentive to the explorer, yet it is clear that the resolution of those now known will afford active and valuable work to whoever undertakes it. It being admitted that the study of Lichens is difficult, still with such aids as I have mentioned, and ready access to the increasing herbariums and literature of the subject, the obstacles and objections disappear rapidly,—it being supposed that one pursues the subject *con amore*.

147 California Ave., Chicago, Ill.

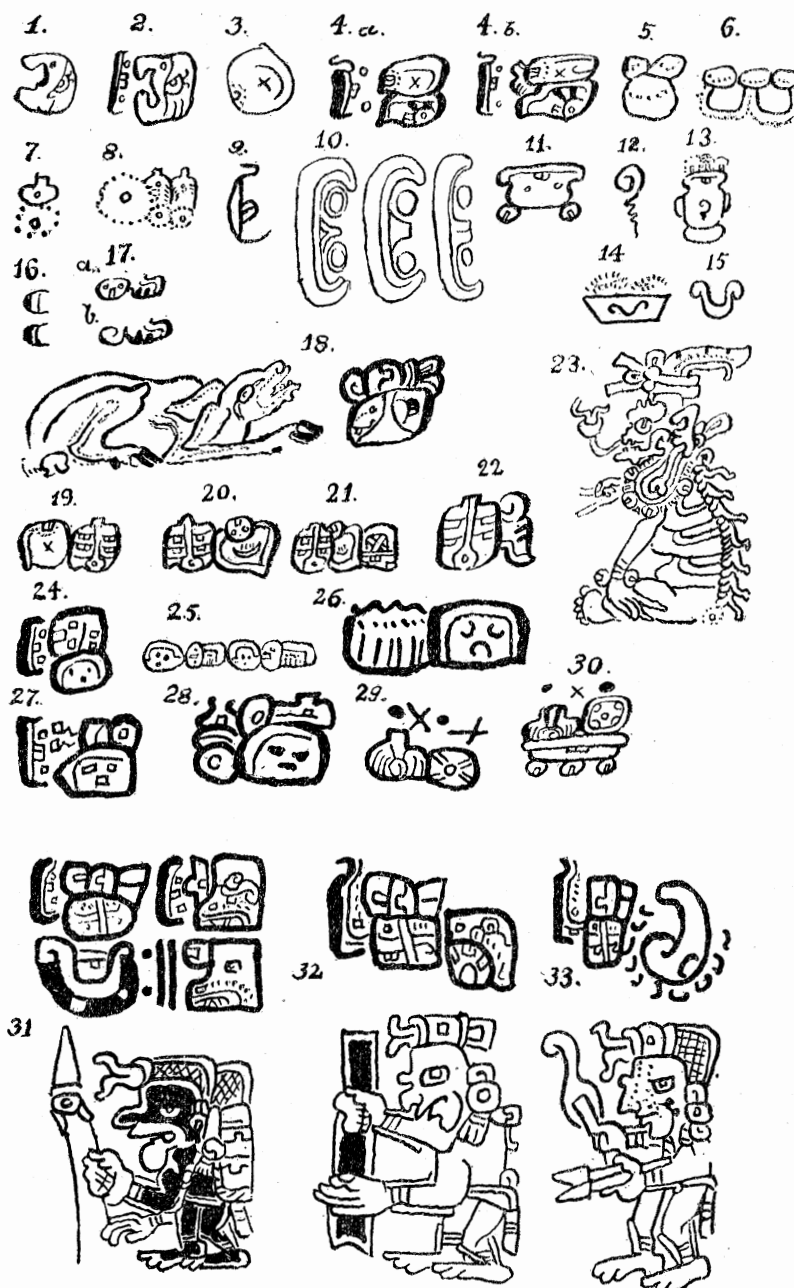


## LETTERS TO THE EDITOR.

## Does There Really Exist a Phonetic Key to the Maya Hieroglyphic Writing?

IN No. 494 of this journal Professor Cyrus Thomas attempts to give a key for the interpretation of the Maya hieroglyphic writing, taking as a guide and starting-point Bishop Landa's well-known alphabet. It is not for the first time that in this way an interpretation of the Maya Codes has been attempted; but as yet most

(Fig. 6) is seen in Dresden 16<sup>c</sup> and Troano 17<sup>\*b</sup>. Landa's *o* (Fig. 7) seems to exhibit the characteristic elements of the hieroglyph of the great red macaw, *mo*, as seen in Dresden 16<sup>c</sup> (Fig. 8). Landa's first *u* (Fig. 9) is a well-known hieroglyphic element, exhibiting on the Copan steles the forms shown in Fig. 10, and undoubtedly conveying the idea of a face, *uich*, perhaps of a bird. The same hieroglyphic element frequently occurs on the neck of the food dishes and drinking cups (Fig. 11), probably on account of the face with which the Indians used to ornament that part.



scientists were of the opinion that these attempts failed to give a satisfactory result.

The hieroglyphs given as letter symbols by Bishop Landa without doubt possessed a certain phonetic value. For instance, Landa's first *a* (Fig. 1) is the head of the turtle, *aac*, represented by a quite similar hieroglyph (Fig. 2) in Codex Cortez, 17<sup>a</sup>. Landa's *cu* (Fig. 3) is the same hieroglyph as that of the day *cauac*, and conveys the ideas of the cloud and of heavy things, as, for instance, a stone. It is an essential element of the hieroglyph (Fig. 4) which expresses the idea of carrying a load on the back, *cuch*. Landa's *ku* (Fig. 5) is the hieroglyph of the bird named "quetzal" by the Aztecs and *kukul* by the Mayas. The sign of this bird

Landa's second *u* (Fig. 12) and hieroglyphic element, which is also seen in the sign of the day *cib*, occurs on the jars filled with spirit-liquor (Fig. 13). It appears to be a modification of a similar design on the Aztec drinking cups (Fig. 14). The latter refers to the *ome toch* symbol, that is, the semi-lunar curved and hook-nosed ornament of the *Totochtin*, the wine gods (Fig. 15). This element therefore, seems to convey the idea of drinking, *uuk*. At last, the sign of aspiration given by Professor Thomas (Fig. 16) is certainly not a "Spanish fabrication," but it is Brasseur de Bourbourg's fabrication, since it is not seen in Landa's text. It has been added to the text by Brasseur de Bourbourg's wholly arbitrary decision. See the photographic reproduction of the

page in question in the publication of Landa's text procured by D. Juan de Dios de la Rada y Delgado. In the hieroglyphic writing the element Fig. 16 occurs as a substitute for the element Fig. 17. The latter, probably, is intended to render the head and the wing of a bird.

It is quite probable that in Landa's time the Mayas used to write in the manner indicated by Landa; we observe the same in the Mexican area. At a certain time after the conquest the Indian writers were inclined to restrict the phonetic value of their old hieroglyphs, in order to write with them in the same manner as the Spaniards did with their respective hieroglyphs. Compare the so-called Codex Vergara of the Aubin-Goupil collection. But this was not so in ancient times. Certainly there existed in the Maya writing compound hieroglyphs giving the name of a deity, a person, or a locality, whose elements united on the phonetic principle. But as yet it is not proved that they wrote texts. And, without doubt, great part of the Maya hieroglyphs were conventional symbols, built up on the ideographic principle.

In order to illustrate the combination of his letter symbols, Professor Thomas gives a few interpretations of groups of compound characters.

This first group (see above, p. 45, Fig. 2) contains in the second hieroglyph (reproduced in my Fig. 24) the elements given by Landa (Fig. 25) as expressing the sounds *l*, *e*, i.e., *le*, the lasso, the sling; and, indeed, in the figure below a turkey is seen hanging in the sling. I do not venture to settle the question by giving an explanation of this hieroglyph. I will only remark that the second element of this sign, that given by Landa as expressing the sound *e*, occurs in various compound hieroglyphs (see Figs. 26-28). In all these cases the action represented refers to handling a rope or to working up thread. Fig. 26 (taken from Codex Troano 31\*<sup>b</sup>) refers to handling the rope trimmed with thorns that the penitent used to draw through the pierced tongue (see the Relief of Lorillard City, published by Charnay). Figs. 27, 28 (taken from Codex Troano, 11\*) refer to weaving and embroidery. It would be a curious coincidence that the words expressing these different actions should all contain an *e*, while considering the idea expressed, the coincidence is a given one.

Considering the third hieroglyph of this group—which is indeed that of the turkey, *cutz* (see Fig. 19), one is in like manner induced at the first glance to think of a phonetic constitution. For the first element is that of the day *cauac*, given by Landa (Fig. 3) as expressing the sound *cu*. And the second element—wanting in Landa's as well as in Professor Thomas's list of letter glyphs—would seem to record the sound *tz*, because it renders the conventional design of a headless carcass or skeleton, *tzietzac*, seen from behind, or in front, with its ribs and the anal opening. Compare the Fig. 23, the design of a skeleton (the death-god) seen "in profile." Nevertheless, it would be a hasty conclusion to proclaim as established and beyond doubt the phonetic constitution of this hieroglyph. For the same element of the skeleton occurs in other hieroglyphs, expressing things the names of which do not contain a trace of the sound *tz*. Fig. 20 is the hieroglyph of the dog, *pek*; Fig. 21, that of the dog of the heaven that carries the lightning; Fig. 22 is the hieroglyph of the month *kan-kin*, "the yellow (or ripe) sun."

But it is principally the first hieroglyph of the group in question that rouses the gravest doubts about the rightness of Professor Thomas's interpretation. The whole group forms part of a series of representations, filling the upmost division of Plates 24\*-20\* of the Codex Troano, and recording, undoubtedly, the capturing of animals. The series begins with the prey-gods of the five regions. These are followed by various representations showing the hunting god—with a captured turkey under the arm, or holding a bag, or armed with spears and throwing-stick (Fig. 33); the black god (Fig. 31 = *Ekchuah*?), and different captured animals, an armadillo (?) in the trap loaded by heavy stones, a turkey seized by the snare, a deer seized by the snare, a deer impaled on the pointed flint erected in the bottom of the pit, a pizote seized by the snare, and a turkey entangled in the hunter's net. Each figure is accompanied by a group of four hieroglyphs (as a rule). The first hieroglyph is the same in all the groups (see Fig. 2, page 45, and my Figs. 31-33), and undoubtedly refers to the action of capturing.

This action is clearly indicated by the form of the hieroglyph that exhibits the head of the victim with the bloody, empty eye-hole, the conventional symbol of sacrifice. This head is held within a sling, the knot of which is seen on the summit. Compare the more accurate design of this hieroglyph in Fig. 18, taken from the Dresden Codex 60\*. In this hieroglyph all is figurative and ideographic; no trace of phonetic constitution can be observed.

The fourth hieroglyph of the group (Fig. 29) is interpreted by Professor Thomas as the second day of the month *yax-kin*. But this is obviously erroneous. There does not exist a numeral designation with crosses between the dots. Fig. 29 seems a variant of the hieroglyph seen in Fig. 30 placed on a bowl. In the latter hieroglyph, the second element signifies *kan*, the yellow color. It is replaced in Fig. 29 by the element *kin*, the sun. The hieroglyph Fig. 30—which in a former communication was interpreted by Professor Thomas as signifying "moisture"—occurs on different pages of the Dresden Codex among the figured representations of offerings (turkey, lizard, fish, deer). Undoubtedly it means an eatable thing, perhaps honey.

I do not enter into a discussion of the second sample given by Professor Thomas (Fig. 3, p. 45), because I find nothing in it that might impel me to accept the translation given by him.

As to Professor Thomas's third sample (Fig. 4, p. 45), I agree with him that the boards covered with the hieroglyphic design of the day *cauac* may be intended for "wood" or "wooden." The same board is seen in Troano 12\*<sup>c</sup>, but fitted with a twisted handle on its surface. Here the first and fourth hieroglyph of the group are also seen; the second one is wanting. Variants of the first hieroglyph occur in Troano 35\*<sup>a</sup>, 35\*<sup>b</sup>, 34\*<sup>b</sup>, and Cortes 21\*<sup>a</sup>, where the figure below shows the god beating a drum. Professor Thomas's explanation, *mul-cin*, "collect together," is merely hypothetical. The same applies to the fourth hieroglyph. It is the same as that given by Landa as expressing the sound *x*. It is materially identical with that of a well-known deity exhibiting in his face the same characteristic design as the face that forms the essential part of this hieroglyph. In Troano 11\* this hieroglyph accompanies the elements which seem to express the action of weaving. And on the two contiguous plates, Codex Troano 35\* and Cortes 22, it is connected with red numerals and forms a row alternating with rows of various offerings. It is scarcely probable that in all these cases the reading *xaan* should correspond to the matter expressed.

The problem of the Maya writing is a difficult one. I cannot convince myself that the list given by Professor Thomas as letter glyphs acts as a key to its interpretation. For the samples of translation he adduces are not forcible, and include misunderstandings. In my opinion, in the present state of things it would be far more appropriate to point out the real meaning, as to the matter expressed, of each hieroglyph. The determination of their phonetic value will then follow, and consequently will then be done with much more accuracy.

DR. SELER.

Steglitz, Germany, Aug. 7.

#### The Fundamental Hypotheses of Abstract Dynamics.

IN Professor MacGregor's interesting paper "On the Fundamental Hypotheses of Abstract Dynamics," the suggestion is made that a fourth law of motion should be added to the three laws of Newton. The proposed law is, in effect, that the magnitude of the stress between any two particles depends solely upon the distance between those particles. Combined with Newton's third law, the new law is thus stated:—

"Natural forces may be considered to be attractions or repulsions whose magnitudes vary solely with the distances of the particles between which they act."

The reason assigned for introducing this law is that "the fundamental hypotheses of dynamics should either include" the law of the conservation of energy "or give it by deduction." This reason seems hardly sufficient. In order that the law of the conservation of energy may be true it is not necessary that the stress between two particles shall depend solely upon the distance between them. It is necessary only that "the work done during any change of configuration of a system of particles acted upon

by natural forces" shall depend "only upon the changes in the positions of the particles, and not upon the paths by which, or the velocities with which, they have moved from the old positions to the new."

Now let  $P$  denote the magnitude of the stress between any two particles of a system and  $r$  the distance between those particles; then  $Pdr$  is the work done by this stress during an infinitesimal displacement of the system. The work done by the stresses between all particles of the system during a finite displacement is  $\sum \int Pdr$ , in which the summation is extended to all pairs of particles and the integration covers the whole displacement of the system. Now if  $\sum Pdr$  is the differential of a function of the quantities  $r$ , the value of the integral will depend only upon the initial and final relative positions of the particles. But the assumption that each  $P$  is a function of the corresponding  $r$  only (in accordance with the proposed fourth law) is only one of many possible assumptions, any one of which would make  $\sum Pdr$  the differential of a function of the quantities  $r$ . The mathematical statement of the condition that  $\sum Pdr$  shall be a perfect differential is given in treatises on "Differential Equations."

It thus appears that the principle of the conservation of energy does not require the truth of the proposed fourth law. The law may be true nevertheless; but it may well be questioned whether its truth is established with any such degree of probability as would entitle it to rank with the laws of Newton as a fundamental hypothesis of dynamics.

L. M. HOSKINS.

Madison, Wis., Aug. 16.

#### The Black-Knot.

ON p. 10, Vol. XX., of *Science* appears an instructive chapter on "Black-knot," a fungous disease of the plum and cherry, of much trouble to cultivators. A point of additional scientific interest is that this fungus illustrates a principle long since presented by the writer of this, that nature does not place species where it is for the best interests of the individuals of the species, but generally has some ulterior purpose not always apparent to us who are eager to uncover her intentions. For instance, there are numberless trees and shrubs that struggle along in swamps, and are rarely found elsewhere, and these have come to be known as "swamp-lovers," but close observation has shown that the same tree or shrub will thrive immeasurably better when removed to dry ground.

On the writer's grounds is a specimen of *Clethra ulnifolia*, fifteen feet high and as much wide on an especially dry spot, and growing with a luxuriance rarely seen in the swampy spots where nature has located the plants. Some reason has been found for the appearance of these plants in swamps and not in dry ground naturally in the fact that the seeds will not sprout in dry, but only in wet ones. It looks like a *fiat* of nature. "Though you would like to grow in dry places you shall not. Something must grow for my purposes, in swamps, and you have to do it." They can only be found where the seed will sprout.

It has always seemed to the writer that it was one of the weaknesses of many discussions in the study of development, that it was generally from the individual standpoint. Nature cares only for the individual, therefore questions of nutrition, fertilization, and others are all viewed in their relation to the plant's "struggle for life." It seems rather that nature cares but little for the individual, and stands ever ready to sacrifice the whole stock when it interferes with some purpose, which we have seldom been able to fathom.

Coming to the black-knot on the plum and cherry, we have here a destructive American species *Sphaeria (Plowrightia) morbosa*, of little injury in its native state, thriving amazingly when it can get as a host-plant the European domestic plum or European morello cherry. It thrives in these cases with a vigor it never shows at home. I have seen it in many parts of the east on the wild dwarf choke-cherry, *Cerasus Virginiana*; in Colorado on its close ally *Cerasus demissa*; in North Carolina sparingly on *Prunus chicasa*; and in the White Mountains on the red cherry, *Cerasus Pennsylvanica*. Recently in driving through various localities on Mt. Desert Island, it was seen on the latter much

more abundantly than in any of the former cases noted; but never anywhere with the amazing destructiveness it presents in these garden representatives of foreign species. In Pennsylvania, and probably other States, the cultivated cherry has been wild for over a hundred years. It is abundant, and in some cases so numerous as to be the chief element in a piece of woodland. But though it is evidently the foster-child of the cherry and not the plum, it prefers the plum and the sour cherry. The knot is rarely found on the wild cherry trees of the sweet cherry species. For all its long hereditary cherry taste, it rushes to the plum and the morello with as much avidity as if long-continued "environment" had induced the love.

It seems to be forgotten in many discussions of the black-knot that it is an American parasite, and that it may be found in quantities everywhere that the botanists look for it. When, therefore, the State of New York tries to "stamp it out" by legislating against garden trees affected with the fungus, it seems like bailing out the ocean with a bucket. Of course, cutting down and burning destroy many spores, but the wild nests send forth myriads of young to take the places of the domesticated foes destroyed.

THOMAS MEEHAN.

Germantown, Pa.

#### Hectoring a Hawk.

EARLY one morning in August, while concealed in the grass and bushes of a White Mountain meadow, I saw an interesting encounter between a sharp-shinned hawk and a number of blue-jays and pigeon woodpeckers. Four of the woodpeckers were quietly preening themselves in a dead pine by the lake shore, when suddenly a small and beautifully proportioned hawk dashed into their midst. They scattered shrieking, and found shelter in a fringe of woods near by. Their cries brought a kingbird to the spot, and the hawk was promptly attacked by the pugnacious fly-catcher and compelled to follow the flickers into concealment. The kingbird, satisfied with routing the hawk, hovered away over the meadow out of sight, and not long after the hawk reappeared and perched in the dead tree.

From time to time one or more of the woodpeckers came back to the tree and were at once charged by the hawk. In each instance they showed superior speed and escaped by their rapid flight. Their noise attracted the attention of a flock of about twenty blue-jays, and presently the blue-winged pirates came sailing over the meadow by twos and threes. As they neared the dead pine the hawk darted downward after their leader. The jay plunged quickly into the bushes, uttering wild cries and squawks, which were re-echoed by his companions. The hawk returned to the pine squealing pettishly, and the jays closed in upon him. They scaled the lower branches of the dead tree; they capped the neighboring maple saplings and alders; they watched for chances to brush past the hawk on his perch, and they assailed him with all the invective of their ample vocabulary. They threw themselves into the sport, as they seemed to regard it, with all the energy of boys playing "short fox."

The hawk took the matter much more in earnest; for he was hungry, and striving for a breakfast. Again and again he shot from the lofty branches of the pine, aiming first at one jay, then at another. By and by all the flickers returned, and added to the confusion by their cries and rapid excursions around the tree. The hawk in several instances seemed to lack but a single wing-beat of success, but the hour drew on without his making a capture. He grew weary. His plumage showed the chafing of the bushes. He chose lower and lower branches for his rests, and finally his sallies seemed directed more towards clearing the tree of noisy birds than to the capture of any one of them. At last he abandoned the dead pine and perched in trees having foliage. The jays followed him jeering, and he shifted his ground slowly until he gained the woods and disappeared. Then the jays crowded into the lower branches of the pine, hopped up from limb to limb until one after another gained the summit, and proved to the whole meadow that they had won the battle and fairly worried the hawk away.

The drama seemed to me to be significant in two ways; first, as

proving the daring of the jay in dealing with the most audacious of the bird-destroying hawks; second, in showing the assistance which an expert hawk, or a pair of hawks hunting together, must gain from the inclination of the jays and woodpeckers to hector them instead of seeking safety in retreat. The advantage which the owl enjoys in drawing other birds around him is well known, but it is not often that so good an illustration is given in the case of the hawk.

Chocorua, N.H., Aug. 20.

FRANK BOLLES.

#### Tornado-Whirls in the Upper Clouds.

THIS morning I witnessed what seemed to me a very interesting and unusual phenomenon, which may be worthy of record. I noticed that a number of light flock clouds, moving north-east in the upper atmosphere, became, on reaching a certain small well-defined area, very ragged, and assumed the characteristic tornado forms. Many looked like jagged craters, reminding me strongly of the photographs of sun-spot whirls; some were honey-combed, and all were greatly torn. In the course of some ten minutes' observation, I saw at least a dozen such tornado-centres in cirro-cumulus, detached clouds floating almost directly above me. Such appearances in the lower clouds I have often observed, but this is the first time I remember seeing the upper clouds disturbed in this manner. The wind at the time on the surface of the earth was a forty-mile gale from the south-west, and there were frequent dust-whirls.

HIRAM M. STANLEY.

Marquette, Mich., Aug. 18.

#### The Brutal Dove.

TWENTY-ONE years ago (Aug. 14, 1871), a mature, male dove flew into the house of Mr. Paul Closius of Chicago, and soon became quite domesticated. "Old Tom," as he is called, was rescued from the great fire of the following October, and later was given a female mate, which he pecked to death.

Thinking that it might be an instance of incompatibility, he was given another, which he tormented, neglected, and abused, until she also perished.

Naturalists are aware of the sentimental error which typifies gentleness in the dove, and have often remarked its ferocity. This instance also confirms the belief that doves are long-lived.

S. V. CLEVINGER.

Chicago, Aug. 17.

#### BOOK-REVIEWS.

*Temperament, Disease, and Health.* By FRENCH ENSOR CHADWICK. New York, G. P. Putnam's Sons. 85 p.

A REAL service is rendered science by those who emphasize the individual as well as environmental side of pathology. The tremendous development along certain lines of modern pathology should not be allowed to obscure the fact that predisposition of the organism is as potent a "cause" of disease as virulence of the germ.

The author of this book avows himself a special pleader on the very first page: "This little book is written primarily to put forward two ideas: First, that there is associated with temperament a specific rate of change; second, that the failure to keep up that rate, or, in other words, a failure to have elimination keep pace with accession of material, is the primal cause of organic disease." This thesis is maintained quite consistently throughout the book. "I thus venture to define what is known as 'organic disease' as a failure in rate of change. And, further, that, however associated, bacteria are the resultant rather than the causes of such diseases" (p. 16).

It will not be perfectly obvious to everyone that the phrase "failure in rate of change" brings us much nearer the real problem. The vexatious question will still be asked, Why should there be this failure to obtain adequate elimination of broken-down material? The final solution of this question of temperament must wait for a much deeper knowledge of the individual cell as well as of the cell-complex. Every attempt, however, at an explanation, although necessarily tentative and imperfect in character, serves its purpose in keeping the subject open and in stimulating research.

Errors of statement do not seem to be numerous. One strongly suspects, however, that the Mitchell mentioned on page 33 is no

#### Reading Matter Notices.

Ripans Tabules cure hives.

Ripans Tabules cure dyspepsia.

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other than the Prudden a few lines below. On this same page "Micro-coccus pyogenes amicus" is spoken of as "a bacillus."

Sometimes a curiously involved sentence is met with, as the following: "We are thus not fixed entities, as most of us are apt to consider ourselves; nor have we the gratification of even thinking ourselves here for the formerly supposed seven years at least" (p. 20). An over-critical reader, too, might take exception to the introduction of the personal element in the following: "Following the logic of these views, Dr. Koch's theory as to the possibility of the cure of consumption by an injection of a preparation of what may be called the dejecta of the bacillus of consumption must of necessity be an error, and I would say that I have held this view from the time of first publication of his supposed cure" (p. 68).

*The History of Modern Education.* By SAMUEL G. WILLIAMS. Syracuse, C. W. Bardeen. 12°. 403p. \$1.50.

THIS work consists of a series of lectures which the author has been delivering for some years past as professor of the science and art of teaching in Cornell University. The entire course comprised also an account of ancient and mediæval education; but the part relating to modern times is the only part now published as being more generally interesting than the rest. Mr. Williams begins his narrative with the Renaissance, of which in its bearings on education he gives a brief but excellent account. In dealing with the religious Reformation and its results, he is not so happy; and throughout the book the subject of religious education receives less attention than it deserves. Mr. Williams treats the history of educational progress by centuries, showing what in his view were the leading characteristics of each century and its principal contributions to educational thought and practice; and this account of the general characteristics of the century is followed in each case by a sketch of the most prominent educators that the century produced. Throughout the book the author shows great impartiality and much good sense in his judgment of men and methods; and, what is no small merit in the

present age, he is entirely free from hobbies. Some of our educators talk as if real education came into the world with Pestalozzi and Froebel, and that in the theory and practice of certain "advanced thinkers" of the present day it has reached perfection. Mr. Williams is under no such hallucination. He reminds his readers that time is the only sure test of historic events, and intimates that some of the ideas of the present day may be found hereafter to have no such importance as is now attached to them. Nevertheless, he devotes one of his longest and most elaborate chapters to the leading educational ideas of the nineteenth century, thus bringing his work down to the very decade in which we now live. He takes pains to show, however, that many things that are thought to be specially characteristic of the present age were anticipated by the thinkers and teachers of the sixteenth and seventeenth centuries. Mr. Williams's style is not always so clear as might be wished, and has no great literary merit; but it is generally intelligible, and its moral tone is good. On the whole, these lectures will serve a useful purpose as an introduction to the educational history of modern times.

*Influenza.* By CHARLES H. MERZ, M.D. Sandusky, O. 96 p.

It would be manifestly unfair to expect too much of a "little treatise" that attempts to discuss a very special topic in a very general manner. The book was evidently written to meet the popular interest in its subject, and this fact alone explains perhaps the infelicities, not to say inaccuracies, of expression that are far too frequent on its pages. The history, etiology, symptoms, pathology, diagnosis, and prognosis, complications, and treatment of influenza are discussed with more or less success, the whole leaving a decided impression of hasty construction.

One is somewhat amazed, for example, when one reads, *apropos* of the phagocyte theory, of the odds arrayed against the Darwinian principle: "It is a fight between two forces and the survival of the fittest" (p. 23). On the same page the name of the eminent author of the doctrine of phagocytosis is hardly recognizable under the mask of "Metschini-Koft."

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Anatomy, The Teaching of, to Advanced Medical Students.  
Anthropology, Current Notes on.  
Architectural Exhibition in Brooklyn.  
Arsenical Poisoning from Domestic Fabrics.  
Artesian Wells in Iowa.  
Astronomical Notes.  
Bacteria, Some Uses of.  
Botanical Laboratory, A.  
Brain, A Few Characteristics of the Avian.  
Bythoscopidae and Cereopidae.  
Canada, Royal Society of.  
Celts, The Question of the.  
Chalicotherium, The Ancestry of.  
Chemical Laboratory of the Case School of Applied Science.  
Children, Growth of.  
Collection of Objects Used in Worship.  
Cornell, The Change at.  
Deaf, Higher Education of the.  
Diphtheria, Tox-Albumin.  
Electrical Engineer, The Technical Education of.  
Eskimo Throwing Sticks.  
Etymology of two Iroquoian Compound Stems.  
Eye-Habits.  
Eyes, Relations of the Motor Muscles of, to Certain Facial Expressions.  
Family Traits, Persistency of.  
Fishes, The Distribution of.  
Fossils, Notice of New Gigantic.  
Four-fold Space, Possibility of a Realization of.  
Gems, Artificial, Detection of.  
Glacial Phenomena in Northeastern New York.  
Grasses, Homoptera Injurious to.  
Great Lakes, Origin of the Basins of.  
"Healing, Divine."  
Hemipter, us Mouth, Structure of the.  
Hofmann, August Wilhelm von.  
Hypnotism among the Lower Animals.  
Hypnotism, Traumatic.  
Indian occupation of New York.  
Infant's Movements.  
Influenza, Latest Details Concerning the Germs of.  
Insects in Popular Dread in New Mexico.  
Inventions in Foreign Countries, How to Protect.  
Inventors and Manufacturers, the American Association of.  
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Jargon, The Chinook.  
Jassidae; Notes on Local.  
Keller, Helen.  
Klamath Nation, Linguistics.  
Laboratory Training, Aims of.  
Lewis H. Carvill, Work on the Glacial Phenomena.  
Lightning, The New Method of Protecting Buildings from.  
Lissajou's Curves, Simple Apparatus for the Production of.  
Maize Plant, Observations on the Growth and Chemical Composition of.  
Maya Codices, a Key to the Mystery of.  
Medicine, Preparation for the Study of.  
Mineral Discoveries, Some Recent, in the State of Washington.  
Museums, The Support of.  
Palenque Tablet, a Brief Study of.  
Patent Office Building, The.  
Phyeta Heterostrophia Lay, Notes on the Fertility of.  
Pocket Gopher, Attempted Extermination of.  
Polariscopes, Direct Reflecting.  
Psychological Laboratory in the University of Toronto.  
Psychological Training, The Need of.  
Psylla, the Pear-Tree.  
Rain-Making.  
Rivers, Evolution of the Loup, in Nebraska.  
Scientific Alliance, The.  
Sistrurus and Crotalophorus.  
Star Photography, Notes on.  
Star, The New, in Auriga.  
Storage of Storm-Waters on the Great Plains.  
Teaching of Science.  
Tiger, A New Sabre-Toothed, from Kansas.  
Timber Trees of West Virginia.  
Trachea of Insects, Structure of.  
Vein-Formation, Valuable Experiments in.  
Weeds as Fertilizing Material.  
Will, a Recent Analysis of.  
Wind-Storms and Trees.  
Wines, The Sophisticated French.  
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